AN EXPERIMENTAL STUDY OF SOME OF THE CONDITIONS OF MENTAL ACTIVITY.

By JOHN A. BERGSTRÖM.

Introduction.—The experimental work described in this paper was done at Clark University, at intervals during the two academic years of 1891-'92 and 1892-'93. The first part gives an account of some experiments upon the daily variations in the rate of certain mental processes, with a view to determining whether there is a natural rhythm of mental activity or not. Under the head of constant daily variations, the results have been grouped to exhibit certain types of mental periodicity. Accidental causes of variation have as far as possible been avoided, though such as barometric changes, which the experiments of Dr. W. P. Lombard and pathological observation have shown to be important, have necessarily occurred. The relative variation of a number of different processes has also been studied. The great instability of the nervous system and the numerous causes of variation prevent the results from being as definite as could be desired, but some general facts may be determined with a fair degree of probability.

The second part, in which more satisfactory experimental conditions were possible, consists of experiments upon physiological memory and is thus a distinct topic; but it has certain bearings upon fatigue and nervous activity which will here be especially considered.

The work has been done chiefly under the direction of Dr. E. C. Sanford, but I wish also to acknowledge my great indebtedness to President Hall and Dr. W. P. Lombard and those who have given their time to the experiments.

Methods.—Two classes of experiments have been selected as tests—one being the repetition of old, the other the formation of new associations. To the first belong reading, adding and multiplying of numbers, and the classification of words; to the second, the learning of nonsense syllable and number series and the sorting of cards. For the number experiments use was made of the columns of Bremiker's logarithmic tables. The vertical columns containing the fifth and sixth places of the logarithms are sufficiently irregular in order in the first fifty pages of the book, if four or five pages be discarded, to serve as material for the experiments. The tables have a further advantage in that the horizontal lines dividing the columns into groups of ten make counting of the amount done easy. The two end figures of each logarithm were multiplied or added, and as many of these operations as possible made in a minute. The figures were read either by ones or by threes with the abbreviated form of expression—thus, "seven-fifty-three," instead of seven hundred and fifty-three. Reading by threes was substituted for

reading by ones, because a certain order would often recur, while in reading by threes, the figures were perceived as a combination and no repetition noticed. In the few experiments with the classification of words, uniformly printed newspaper articles were used. The classification was based upon use. A test of the rate of voluntary movement was also employed in a few experiments. The plan was to simplify the ordinary writing movement, as far as possible, so that there should be no considerable qualitative change, and at the same time to make the amount of work done easy to count. Ten parallel lines were drawn across ordinary ruled paper, thus making ten squares on any horizontal line. These squares were filled with five crosses or ten strokes. The number made in a minute was taken as a test. This number could be seen at a glance. A convenient test of the precision of movement may also be found by using this ruled paper. With a fountain pen or a needle point make a dot or a needle prick, and then attempt to put the pen or needle point in the same place. The number of errors will be inversely proportional to the precision. Of course a given movement and rhythm must be chosen. If five dots are made in each square, counting of the errors will be easy.

An inexpensive but accurate timing apparatus was made for use in these experiments. A pointer on the second's axis of a clock dipped into a drop of mercury and thus made the circuit of an electric bell. This gave minute signals. For fractions of a minute, stars with the required number of points could be put upon the second's axis. By putting stars with the required number of points upon the minute axis, signals can be had for any number of minutes. Here large errors would probably come in if the closing of the current was made to depend upon the slow moving minute star. The circuit is, therefore, so arranged that both the minutes and the seconds points must be in their respective drops of mercury simultaneously before the circuit is closed. This makes the signal depend upon the movement of the second hand; and since there is only one point to consider, and not the distance between two successive points, the errors are very small.

The nonsense syllables were learned by the method used by Ebbinghaus. A series usually contained ten syllables. The number-series consisted of thirty digits, learned by threes. The series were read over in a given rhythm and the attempt made to repeat them. As soon as there was hesitation over a syllable, the reading was begun with it and continued to the end, and then an attempt was again made to repeat the series. The series was considered learned at the first perfect repetition. The card-sorting memory experiment consists of sorting a pack of eighty cards into ten different piles. Each pile is to contain eight cards bearing the same word or picture. In subsequent experiments different arrangements of the piles were used, so that as a memory experiment it consists essentially, like the nonsense syllable series or the memory span test, of learning different permutations of the same symbols. Care was taken that there should be no possibility of association or grouping of the words or pictures. As a memory experiment, it is especially valuable for the study of the interference of associations. The time of all the memory experiments was taken by a stop watch.

Only such material was used in the experiments as was of uniform quality and of practically unlimited amount. The comparative uses and value of the different tests is to be judged by the results. As regards errors, the effort was made to correct

them and thus include their value in the total time of the operation. In the experiments upon the daily variations in rate, the general plan was to make experiments every two hours throughout the day. The effect of practice and fatigue for the tests themselves was reduced by preliminary practice to a comparatively small quantity. The influence of the interference of associations in the memory experiments is more troublesome; but at the end of two hours its effect is small, so that great changes must be due to other causes. The results of a number of days were then averaged, giving the constant daily variations. The subject of the experiments was asked to be as regular as possible in his habits, and to follow an average routine of work and rest. This is, of course, an essential requirement. In the majority of cases it was quite carefully complied with. Since large changes in the tests themselves have been eliminated, any such that occur will be due to general changes in the nervous system and the circulation.

Constant Daily Variations.—Table I. aims to give a general view of the changes in rate throughout the day. The figures under morning, afternoon, and evening represent seconds; and for E. C. S. give the average time required for learning three series of nonsense syllables of ten each at those times of day. The rest give the average time required for sorting two packs of cards in the way described. Two similar packs were used by W. O. K., and by A. F. for the first average. In all the other experiments, two different

TABLE I.

	Morning.	Afternoon.	Evening.	No. of Days.
E. C. S.	478.3	544.	573.3	10
M. N.	188.7	197.	214.	11
J. A. B.	211.94	225.66	249.7	7
P. E., Av.	±1.99	±2.07	±2.32	
L. N. B.	225.4	224.3	269.6	11
A. F.	{ 269.4	264.4	288.8	8
	{ 223.49	216.85	244.3	8
W. O. K.	288.4	300.	282.2	6
P. E., Av.	±2.09	±2.48	±3.19	
E. T.	200.34	196.45	193.4	5

packs were used. Differences in manipulation will make it impossible to compare the rates of all the different subjects, but this was constant for any given one. The right hand column gives the number of days from which the average was made. Usually there were four experiments in the morning and three in the afternoon and evening each. The morning average of E. C. S., for example, is made up from forty experiments—or one hundred and twenty series. A low record indicates rapid rate, and a high record the contrary.

The records of three other subjects averaging ten days each, but covering only the morning and afternoon, show no decided difference between these two periods.

The averages show that the rate of the first three subjects diminishes throughout the day, while that of the next two is as good or better in the afternoon, though poorer in the evening. The rate of E. T. increases steadily. W. O. K. makes the best records in the evening, but is poorer in the afternoon than in the morning.

Only the probable errors of the records which do not receive special study in Table II. are given. The daily variations in rate are not of a single type such as would be required if a natural, inherited rhythm of activity existed. The daily rhythm is the resultant of a number of nervous and circulatory influences, which will be discussed after a statement of the results has been made.

Table II. gives a more detailed statement of the records which are adequate as regards number, and regularity of experimental conditions for such treatment. The probable errors—as a measure of the closeness with which each subject adheres to his type and of the reliability of the experiment—are greater than they should be since the effect of practice is added to that of accidental variation. Only a rough correction can be made, however, and the general results can be established without such correction. The effect of practice can be seen in the averages of the successive days. For L. N. B. there is an average decrease of about three seconds per day at first, the last four days being nearly the same. A. F., whose preliminary practice was considerable, shows no decrease of time. The records of M. N. diminish about four seconds the first four or five days, after which the averages are about the same. Those of T. L. B. decrease about one second per day. The effect of practice in these cases has evidently little influence upon the daily curve. The experiments of E. T. extend over only five days, but they were made very carefully and are of special interest, since they represent a distinct type. The effect of practice during the experiment can be seen from the averages of the five days, which are 203.67, 202.28, 202, 195.54 and 184 respectively. The great increase of rate on the fifth day is probably not to be attributed so much to practice as to change in health. With the exception of an experiment in which a subject sorted cards for an hour or more continuously, no signs of fatigue for this experiment itself have been observed. A few trials were made to see if a second experiment would give different results from the first, especially when an unusual record was made. The two records usually differed but slightly, if the effect of the interference of association is taken into account. In another experiment with four other subjects, about as many experiments were made in an hour as were here made in a day, but no evidence of fatigue is to be seen, though the average endurance is probably no greater. The general effect of the interference of associations is to make the first of a series of records shorter than the second, while the rest will not differ much from the second. It would not influence the general curve of the day except with respect to the first record in the morning, which would be shorter than it should

be. The fact that with more intense work the interference is slightly greater is a disturbing element, however, which needs to be taken account of. The amount of the changes and the individual peculiarities of the curves show at a glance that they are not due to small and comparatively constant changes in the experiment itself. The experiment of E. C. S. was performed at three different periods, three days being used the first and second time and four the third. The averages of the records for the three periods stand

TABLE II.

	A. M.				P. M.						
Hours.	7.30	8.30	10.00	12.00	2.00	4.00	6.00	7.00	8.30	10.00	Number of days.
L. N. B.	234.2	218.1	225.8	223.6	215.8	231.2	226.	253.	280.8	275.	11
P. E.	±2.2	±1.8	±2.89	±1.79	±1.76	±3.3	±2.79	±2.	±4.	±4.16	
A. F.	233.4	229.4	216.8	214.3	213.3	211.9	225.3	244.6	236.3	250.4	8
P. E.	±4.32	±4.21	±2.8	±3.8	±2.29	±1.33	±4.3	±3.8	±3.68	±5.8	
Hours.	7.30	8.30	10.00	12.00	1.30	3.00	6.00	7.30	9.30		
M. N.	192.8	185.	183.6	193.4	186.3	197.1	207.6	207.2	222.6		11
P. E.	±4.4	±3.24	±2.7	±2.55	±3.5	±3.5	±3.	±5.	±3.7		
E. T.	198.	205.3	204.5	193.54	197.6	199.3	192.45	201.6	185.2		5
P. E.	±4.8	±4.6	±4.7	±4.48	±2.4	±1.5	±3.48	±3.3	±1.8		
Hours.	7.45	9.00	11.00	12.45	2.00	4.00	5.45	7.00	9.00	10.30	
E. C. S.	452.7	484.7	467.9	508.	531.4	507.7	593.	536.3	578.	605.6	10
P. E.	±12.48	±19.4	±13.44	±16.53	±25.49	±14.24	±20.98	±14.76	±20.47	±15.01	
Hours.		8.00	10.00	12.00	2.00	4.00	6.00				
T. L. B.		179.7	189.4	187.8	190.7	189.3	192.7				17
P. E.		±3.6	±2.1	±2.56	±1.84	±1.33	±2.63				

as one hundred to eighty-six and seventy-nine respectively, but in each of the sets of days there was an increase rather than a decrease of time, showing fatigue rather than practice. The effect of interference was so slight that it can't be demonstrated between successive series. It is believed that changes in the experiment itself are inadequate to account for the great daily variations. A three days' experiment with card-sorting gives variations corresponding with those for the nonsense syllables. The general daily routine differed a little with the different subjects, but in general the hours of sleep were from 10 to 7; breakfast was eaten immediately after the first, dinner after the 12 o'clock, and supper after the 6 o'clock experiments. With the exception of E. C. S. the subjects were between twenty and thirty years of age; E. C S. is a little over thirty. All were members of the psychological department except M. N. and E. T., two ladies, one of whom is a student of medicine, the second a teacher. All were in good health during the experiments except E. T., who had not yet recovered from a severe attack of nervous prostration. The constant daily variations of E. T. will be seen to show the typical symptoms of morning tire and depression after meals, though the rapid rate at all times shows that there is no lack of power to concentrate.

The accompanying chart will give the general picture of the daily variations at a glance. The first curve shows that breakfast and dinner are stimulating. The middle of the morning and afternoon shows lower rates than the beginning and end of those periods. The high records and consequently low rate after supper is probably due to a habit of relaxation at that time. The subject complained of sleepiness. The better record at 10 P. M. than at 8.30 P. M. may be accidental, but is probably due to the fact that 10 to 12 P. M. were found to be good hours for study in college and had been so used. The subject took tea or coffee morning and night, but evidently not in sufficient quantities to give the characteristic effect of meals, since the records after breakfast are low, while those after supper are high. The curve of A. F. does not show any decided effect of meals. The records after meals seem rather to take their proper place in the general tendency of mental activity to increase or diminish. Coffee or tea was taken at each meal. The first two records show a morning depression. L. N. B. was up, on the average, thirty minutes earlier than A. F. and felt wide awake, while A. F. complained of sleepiness in the morning. From 10 A. M. to 4 P. M. there was a steady increase of mental activity. At 6 and 7 P. M. the records are higher, in spite of recreation and supper. The time from 8 to 10 P. M. was usually devoted to study, and the better record at 8.30 P. M. is probably due to the increase of mental tension. Early morning and evening depression, with an intermediate period of constant activity, is the characteristic of this curve. The curve of M. N. is marked by a decrease in rate during the day, interrupted by the stimulating influence of meals. Tea or coffee was usually taken at meals. The curve of E. C. S. shows a decrease of rate from morning till night. The low rate before dinner and supper is a noticeable feature. After supper there is a marked increase of rate. The effect of breakfast and dinner is not certain, on account of the large probable errors, though the average is higher in both cases. The curve of E. T. differs wholly from the rest. It shows an increase in rate throughout the day interrupted by depression after meals. Tea or coffee was usually taken at meals.

Accidental Variations.—At the suggestion of Dr. W. P. Lombard a comparison of all the records with the barometric readings was

FIG. I.

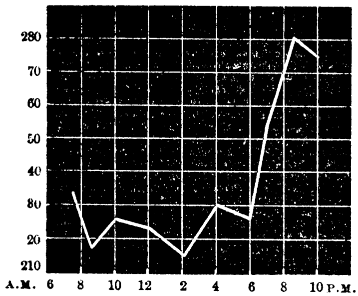


FIG. V.

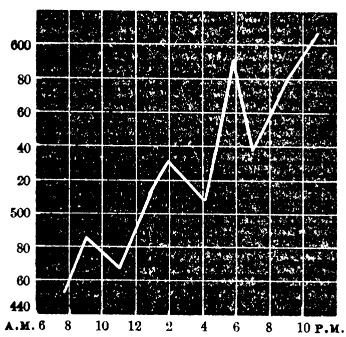


FIG. II.

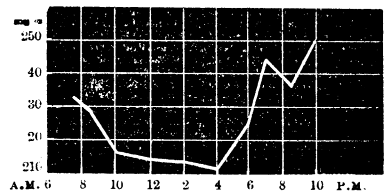


FIG. III.

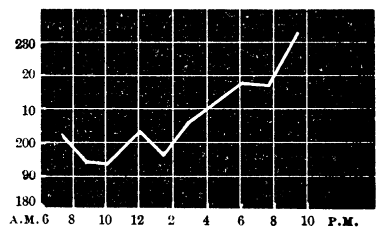


FIG. IV.

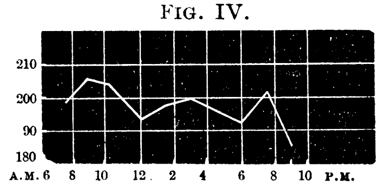
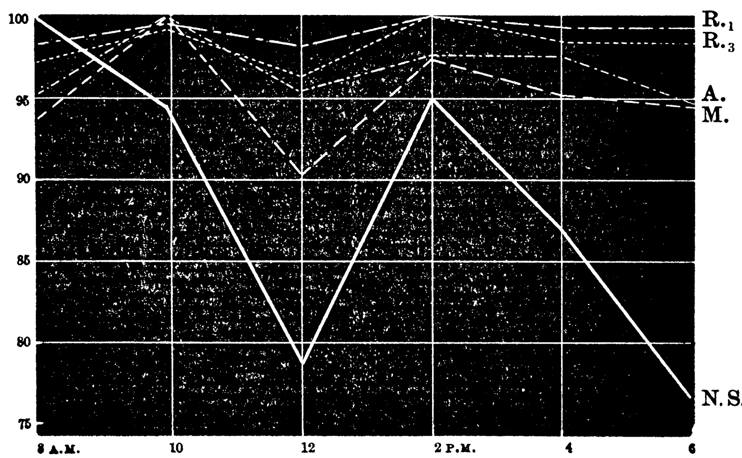


FIG. FOR TABLE IV.



made. The method adopted was the following: The curve of the average representing the constant daily variations was plotted over the curve of variations for each day. The state of the barometer, whether rising, falling or stationary, was then marked upon the curve for the day. The constant barometric variations, with maxima at 10 A. M. and 10 P. M., and minima at 4 A. M. and 4 P. M., are very slight in this latitude and can only be demonstrated by the average of a large number of days. The great fluctuations are irregular. The direction of change of the daily curve from the average is of course either the "same" or "above" or "below." The classification of these changes of direction under rising, falling and stationary barometer gives the results in Table III. The first two subjects evidently show no dependence upon barometric

TABLE III.

		Rising.	Falling.	Stationary.
	Above.	14	19	12
	Below.	15	19	11
L. N. B.	Same.	13	9	3
	Above.	13	13	3
	Below.	14	11	4
A. F.	Same.	6	4	4
	Above.	7	21	17
	Below.	17	13	13
T. L. B.	Same.	3	1	9
	Above.	9	22	8
	Below.	13	12	8
M. N.	Same.	1	4	6

changes, since there are about as many records above as below the average for the various states of the barometer. T. L. B., whose records are very variable, though showing no constant daily changes from 8 A. M. to 6 P. M., gives a preponderance of low records for rising and high records for falling barometer. The same is true of M. N. in a less degree. There would seem to be a correspondence between rapid rate and rising barometer and poor rate and falling barometer in the last two cases. Observations sufficient to establish a safe independent proof of the influences of barometric changes should be more numerous than those we have, on account of the improbability that such slight changes of quality and distribution of the blood as ordinary barometric fluctuations probably produce, will have any effect on the mental processes. The results may be taken to support the view that some people are largely influenced by barometric variations, while others are not. Most of the experiments were taken in March, April and May, so there were in general no extreme temperature changes.

The charts of a Draper self-registering barometer were kindly supplied for the work by Mr. Martin Green, of Worcester.

Other causes of variation occur only a few times. They are mainly external and such as tend to concentrate or distract the attention—though some are of a purely physiological nature.

The Relative Variation of Different Processes.—Table IV. gives the result of an eleven days' experiment by the writer. Six experiments were made a day, beginning with 8 A. M. At each experiment the pulse rate was noted; and then the rate of reading, adding and multiplying numbers, and of learning nonsense syllable series, was obtained in the way described above. Four ten-syllable series were learned each time. A uniform daily routine was maintained—8 to 10, reading of psychological literature; 10 to 12, reading and listening to a lecture; 12 to 2, dinner and recreation; 2 to 4, laboratory work; 4 to 6, walk and recreation. The experiment required from fifteen to twenty minutes each time. For the sake of ready comparison the results have been reduced to percentages, the quickest record being counted 100. The numbers for the nonsense syllables are the inverse of the times required, so as to correspond with the other records. Since the question is here how much can be done in a given time, and not, as in the former case, how long will it take to do a fixed amount, the meaning of high and low records is the reverse of that in the previous tables—a high record meaning rapid rate and a low record poor rate. At the best time it required, respectively, 0.325, 0.346, 0.857, 0.907 seconds for reading a number in a set of three, by ones, and adding and multiplying two together, inclusive of pronunciation; 110 seconds were required for a ten-syllable series at this stage of practice.

The right hand column M. V., which gives the mean variations of the six averages of each test, shows the relative amount each changes under the same physiological influences. With respect to the amount of variation, the tests may be arranged in a series, beginning with the reading of numbers by ones and ending with the nonsense syllables. The same series will be formed if they are arranged in the order of complexity or difficulty, the time required being taken as a measure of the difficulty. An exception must be made of reading by threes, though this is probably a more complex process than reading by ones, in spite of the fact that less time is required per figure.

Some facts may be found here for deciding what test would be most serviceable in similar experiments, and how far variations in the rate of certain processes may be taken as an index of changed

TABLE IV.

	A. M.			P. M.			
Hours.	8	10	12	2	4	6	M. V.
$R._1$	98.27	99.71	97.92	100	99.13	99.2	.765
P. E.	±1.10	±.59	±.97	±.80	±.61	±.85	
$R._3$	97.23	99.29	96.04	100	98.59	98.37	1.02
P. E.	±1.21	±.82	±1.10	±1.52	±.92	±.76	
A.	95.29	100	95.7	97.57	97.57	94.86	1.55
P. E.	±1.53	±1.43	±1.67	±1.68	±.90	±2.	
M.	93.8	100	90.32	97.43	95.01	94.4	2.37
P. E.	±1.01	±1.04	±1.84	±1.22	±1.43	±.87	
N. S.	100	94.24	78.5	94.9	86.88	76.06	7.95
P. E.	±3.16	±3.98	±3.54	±3.72	±3.36	±5.10	
Pulse.	66.9	62.2	62.6	66.8	65.4	65.2	

$R._1$ and $R._3$ refer to reading numbers by ones and by threes respectively; A. and M. to multiplication, and N. S. to the nonsense syllable series.

physiological conditions. If only very slight, or even no change could be observed in the simpler processes, very striking variations might, nevertheless, be found in the more complex ones. These facts are in harmony with the common experience that easy work can be done almost any time, while the most difficult work can only be done well at rare intervals. The easier tests might be difficult for persons weakened by sickness and so give considerable variations. Reading of numbers would perhaps be sufficiently difficult in many cases to indicate the more considerable fluctuations. With opportunity for plenty of experiments, the most difficult test will give the most striking and satisfactory results; but with only a few experiments no conclusions could be drawn on account of its great irregularity. The greater regularity of the simpler tests makes fewer experiments necessary, but the variations will be very small if demonstrable at all.

A glance at the cut will show that with one exception the variations of the different processes are the same throughout the day. The exception is the nonsense syllable record at 10 A. M. This does not, like the rest, show an increase of rate at 10, though it is not

certain that it shows a diminution. The time between 8 and 10 was occupied with reading, and the result is probably due to a differential effect of this kind of mental work upon the two kinds of processes. The interference of associations is quite marked in the case of the subject. This would account for a small part of the difference, but it should theoretically only affect the first series at 10 A. M., making that a little longer than it would be without the record at 8 A. M., so that the influence would be much too small by itself to account for the result. The variations of the processes under the influences of rest and dinner, fatigue and exercise, it will be seen, are the same. A test of the matter, by exclusion, between 8 and 10 A. M., was not made on account of lack of time. The increased mental excitement at 10, shows itself in greater rapidity of well learned processes, but also by the presence of unconscious tendencies to distraction, which make themselves known by increasing the time of learning the nonsense syllables. At 12 M. there is a decrease of all the processes, but at 2 P. M. a considerable increase, after dinner and rest. Four and 6 P. M. again show poor records.

The fact that easy operations hardly change at all, while the more complex show considerable variation, was the first result noticed in the preliminary experiments, and formed the basis for further work. In a five days' experiment by two subjects, the following times were required for sorting an ordinary pack of playing cards into four piles at the times of day designated. Three records were made at each experiment. Though the process is far more complex than simple reaction, the changes in it are very small:

TABLE V.

	A. M.		P. M.			A. M.		P. M.	
	9	11	4	10		9	11	4	10
	53.33	50.66	51.2	53.93		53.1	51.4	51.7	57.1
E. C. S.	19.95	19.81	19.3	19.1	J. A. B.	22.9	21.7	21.6	24.9

The first row gives the hour of the experiment; the second, the average time required for the sorting; the third, the time for distribution into four piles *without sorting*. The records of E. C. S., with the nonsense syllables, were taken at a different time, but otherwise under the same conditions, so that a rough comparison may be made. It is evident the ability to learn nonsense syllables changes much more during the day than the ability to sort cards into so few positions. A similar rough comparison of the variation of card memory test for the writer, gives 2.6% as the mean variation of the six records of the morning and afternoon. This makes it stand between multiplication and nonsense syllables in its sensitiveness to variations, though much nearer multiplication than the other. Another series of experiments of four days, besides preliminary practice, with the classification of words and the test for the rate of movement, gave the following results, which, though taken at another time, correspond quite well with those of Table IV.:

TABLE VI.

J. A. B.

A. M.				P. M.					M. V.
7.15	8	10	12	2	4	6	8	10	
262.5	272.5	278.5	267.75	277.25	270.75	269.75	260.25	260.75	1.97%
120.75	132.	135.	124.	135.5	135.5	140.25	131.5	120.	4.52%

The first line gives the hours, the second the number of strokes per minute, the third the number of classifications made in two minutes. Only a rough comparison with Table IV. is intended. The mean variations in the right hand column show that the classification of words undergoes the greater changes.

The experiment, the first eleven days of which gave Table IV., was continued thirty-two days longer, with various changes in the memory test. On ten of these days the regular occupation from 4 to 6 was changed to exciting physical exercise—ball playing or tennis. The results are shown in Table VII. The memory test was not the same on the different days, since they were made for a different purpose. It consisted, usually, of two number series alternating with two nonsense syllable series, but other combinations occurred. The probable errors cannot therefore very well be calculated, but the extraordinary change in rate does not leave any doubt of its existence. It will be remembered that with ordinary quiet recreation there was, on the whole, rather a decrease than an increase of rate at 6. The intermediate days, on which no exciting exercise was taken, show this also. If the 6 o'clock records are put at 100 and the 4 o'clock records expressed as percentages of them, another proof is obtained that the variation of the more difficult processes is relatively greater under the same physiological changes, which may be either *depressing* or *stimulating*.

Some of the 6 o'clock records were made immediately after the exercise, others after about a half hour, but the effect was nearly the same. In the third series of experiments upon the parallel law for lifted weights, Fechner brought on intense muscular fatigue by lifting nine and one-half pound weights in a rapid rhythm. The exercise aroused his whole system, as a very rapid pulse rate indicated. The number of right cases increased instead of diminished after the operation. In a fourth series of experiments, in which the weights were lifted slowly, though the fatigue operation was performed five times in succession, so as to get a cumulative effect, only a comparatively small increase of the pulse rate was noticed, and the number of right cases after the operation is about the same as that before. It seems not unfair to say that these experiments show that with a general physiological change—in this case of a stimulating nature—there is a corresponding change of the power of discrimination, even if this does not change with local fatigue of the sense organ, which latter fact Fechner had especially in view. The analogy with the experiments reported in the last table is, of course, evident, though in the one case rate, in the other

TABLE VII.

	4 P. M.				6 P. M.			
Date.	$R._3$	A.	M.	N. S.	$R._3$	A.	M.	N. S.
April 4.	210.	73.	70.	322.7	221.	77.	78.	352.3
" 5.	205.	77.	59.	362.7	219.	76.	74.	243.3
" 10.	216.	79.	73.	172.25	228.	88.	67.	150.75
" 11.	207.	80.	56.	217.8	232.	83.	74.	112.8
" 19.	222.	83.	72.	252.8	232.	96.	89.	148.
" 26.	198.	86.	88.	153.5	219.	91.	97.	89.8
May 5.	217.	89.	89.	241.	240.	94.	98.	158.3
" 6.	216.	89.	87.	297.	243.	105.	103.	198.5
" 10.	217.	92.	88.	196.	237.	100.	101.	168.8
" 11.	214.	85.	85.	301.5	235.	101.	103.	220.5
Average.	212.2	83.4	76.7	251.72	230.6	91.1	88.4	184.31
	92.02	91.5	86.7	73.2	100.	100.	100.	100.
Pulse.	67.3				85.4			

$R._3$, A. and M. refer to reading by threes adding and multiplying respectively; N. S., to the nonsense syllable series.

discriminative sensibility, is measured. The stimulating effect of exciting physical exercise is a matter of common experience. A mathematician, who kindly replied to a circular of questions regarding habits of mental work, finds the first hour and a half after a game of tennis especially valuable for original work. Fechner attributes the increase of the power of discrimination to the acceleration of the circulation. As will be seen later, certain changes in the nervous system probably coöperate with this to produce the result.

Tables IV. and VII. give some data for a study of the relation of the rate of mental work to the circulation. In experiments with the plethysmograph and balance, Mosso and others have shown that any mental activity from a simple sensation to the solution of a problem is accompanied by an almost instantaneous alteration of the circulation. The theory of Mosso that, in this case, the cerebral

circulation increases while the peripheral diminishes, seems to be based upon too few observations. According to Féré, moderate and pleasant mental activity may be accompanied by an increase of the peripheral circulation, while intense or disagreeable mental effort is marked by a depression. The same author says the tipping of the balance toward the head does not necessarily show an increase of the cerebral circulation, since the majority of organs which express the emotions are on the head side of the centre of gravity. Direct tests of the intracranial blood pressure make it probable, however, that mental activity is accompanied by an increase of cerebral circulation, though the actual amount of blood in the brain does not change very much, since the skull is a closed cavity. M. Gley found a dilation of the carotid, a contraction of the radial artery, and an increase of one to three pulse beats per minute during hard mental work. Leumann, in some experiments upon boys in a gymnasium, found that the normal scansion of poetry was in direct ratio to the pulse frequency. He also explains the so-called apperception rhythm of minimal differences of sensation and the periodic umbrations of mental images as due to the influence of respiration upon the circulation. This subject will be referred to again in the discussion of the results.

A comparison of the rapidity of the pulse with the rate of the mental processes, in Table IV., will show that there is no necessary correspondence between the two. The pulse rate falls from 8 to 10, while the rate of all the processes but the nonsense syllables increases. The pulse rate is about the same at 12 as 10, but the rate of all the processes has diminished. For the afternoon there is a fairly close correspondence. The lack of correspondence between the pulse rate and mental activity is much more striking in the following experiment, which, though it failed in its direct aim, has several points of interest. The object was to study the effect of one kind of work carried on till considerable fatigue appeared, upon another kind of work entirely different. In experiments with the ergograph, Mosso found that Dr. Adduco made a better record in the middle of a four or five hour period of exciting mental work than at either the beginning or end. No measure of the mental work is made, so it is impossible to say whether the increase and decrease of the test work coincided with similar changes in the main work. The object here was to measure both kinds of work, substituting tests of the rate of mental processes for the ergograph record. The translation of German was the main work. Adding, multiplying and reading of numbers and the experiment upon the rate of movement were the test work. A record with the tests was made at 8, 10 and 12 in the morning. The rest of the time from 8 to 12 was divided into eight equal parts by the stroke of a bell. The subject (C. H. J.) translated with the greatest possible rapidity, marking off at the strokes of the bell the amount done. The experiment was made on twelve days, but only the last seven were under proper conditions. Scientific German had been taken up as a study only a few weeks before. Wundt's "Physiological Psychology" was the book read. The number of words for which the dictionary had to be used was also noted. It was supposed that with advancing fatigue they would become more numerous. The experiments were made the first part of August.

TABLE VIII.

No. of lines.	23.8	25.1	29.9	25.9	27.9	24.2	28.1	29.4
P. E.	±2.98	±2.46	±1.61	±3.39	±4.78	±.95	±1.54	±3.03
Unknown words.	7.	7.1	6.7	7.6	7.6	7.4	6.9	6.9
P. E.	±.39	±.35	±.44	±.52	±.46	±.42	±.53	±.44

The first horizontal row gives successively the number of lines translated in each of the eight equal divisions.

The table below gives the rate of the test processes at the times mentioned. The probable errors of the tests are given in the right hand column at the given hours.

TABLE IX.

8 A. M.		10 A. M.		12 M.		
71.3	±2.3	60.7	±1.82	53.	±.39	Pulse.
97.4	±1.46	101.2	±1.26	102.6	±.86	Addition.
64.4	±.74	63.3	±.69	61.7	±1.51	Reading.
81.	±.79	81.	±1.08	81.6	±.62	Multiplication.
260.6	±1.27	268.4	±1.68	268.7	±1.86	Movement.

A possible source of error is that there may have been an unconscious allowance for the amount which had to be done. Toward the end of the four hours' work there was a severe feeling of fatigue, but there is no corresponding diminution of rate either in the main work or test work. After dinner there was a disinclination to effort of any sort. A glance at the records will show that the rate of the main work and the other processes is nearly the same throughout. During the first five days, a recess of fifteen to twenty minutes was taken between 9 and 10. With that routine there was an increase of all the processes at 10, and a diminution at 12, as in Table IV., and there was no feeling of fatigue after dinner, but work was usually taken up at once. The noticeable thing is the great change in the pulse rate, namely, from 71.3 to 53, while there was no change in the rate of the mental processes. The high pulse rate at 8 A. M. is due to the breakfast which has just been eaten, and there is a similar increase after dinner. While an increase of mental activity probably causes an increase of blood flow to the brain, mental activity can evidently not be said to vary with the pulse rate.

Discussion of Results.—The experiments, so far reported, show that the subjects, whose records cover the entire waking period,

have a well marked periodicity of mental activity. There is, however, no *general* type of daily rhythm, and individual differences of the most striking sort occur. The same influences have different effects upon different individuals. The more complex mental processes have relatively greater variations than the simpler. Under the influence of fatigue, rest and physical exercise, the processes studied vary in the same direction. Statistical investigations show that those who are engaged in mental work have generally observed a daily rhythm of power. In connection with other questions, Heerwagen sent out the following: What part of the day do you find mental work easiest? To which, 182 said the morning, 133 the evening, six the afternoon, forty-three noticed no difference, while twenty-eight found it easy at all times. Professor Earl Barnes, in a study of the intellectual habits of Cornell students, received in reply to the same question, sixty-six votes for the morning, six for the afternoon and thirty-nine for the evening. The average student, he says, sleeps eight and one-fourth hours, begins work at 8, but is in doubtful condition, is best at 9, at 10 still in good condition, at 11 he is tired and at 12 is at his worst. He works from 3 to 5 in the afternoon, but in inferior form. After supper he goes to work at 7, and reaches his best at 8. From 9 P. M. he is not at his best, and at half past ten goes to bed. The smallness of the number who chose the afternoon is probably due to the hard work of the morning which all had. Forty-five said their power was uniform from day to day, 109 that it was variable. Three-fourths accounted for the variations by the weather, dry, clear days being approved, dull, damp days denounced. The state of feeling, regular sleep and meals were the causes next in order of importance. Many said they were less able to work after a rest than when they had gotten at it. Thirty-eight said they could get the most rest in an hour's time from exercise, thirty-one from sleep, twenty-three from a walk, six from light reading, four from a bath and three from music. The location of the best hours depends largely upon the hours of retiring and rising. It will be seen that actual measurement and statistics of opinions both show that in a certain number of cases there will be persons whose maximum activity comes at almost any given hour of the waking period. The rhythm of activity may or may not correspond with the actual energy at the person's disposal. Other things being equal, the total amount which can be accomplished decreases as the interval since sleep increases, but the rate of work may be most rapid a little before retiring, as in the case of E. T. and W. O. K. While most persons thus recognize the existence of a daily periodicity of activity, which is of great importance for the quality and quantity of work they can do, this does not conform to any general type, and is, therefore, not an inherited modification of the nervous system. The daily rhythm is the result of a number of stimulating and depressing causes, whose influence habit tends to fix upon the system. Changes of hours of work are often made, though with more or less difficulty.

Some of the answers to the circular sent out by the writer state that a change from evening to morning work had been made. The evening hours were still preferred, but had to be abandoned, because they were "expensive" and sleep was interfered with.

The preceding results do not furnish the material for their own explanation, except in a few points. A brief review of some of the literature bearing upon the subject may therefore be permitted. The hope of finding a thoroughly satisfactory explanation is of course not entertained; but the following facts are suggestive:

Dr. Hodge has demonstrated that nerve cells under electrical stimulation and ordinary fatigue show, for the nucleus, a marked decrease in size, a change from a smooth to a jagged appearance and a darker staining; for the cell protoplasm, shrinkage and vacuolation. The materials of the cells are highly complex and unstable, while the waste products of their activity are more simple and stable. Change from unstable to stable compounds, in this case chiefly by oxidation, is accompanied by the setting free of energy, part of which is used to build up its own substance and part as free nervous energy for the stimulation of muscles and other cells, while another part, as Schiff has shown, appears as heat. Of special interest here is the experimental study by Wundt of the relation of the anabolic and katabolic processes, and the changes of these in sthenic and asthenic conditions of the nervous system. If a strong electrical stimulus is used to test the condition of a nerve during the progress of stimulation from another current of moderate strength, its effects upon the muscle contraction will be reënforced. If a weak test stimulus is used, it is usually suppressed. This is interpreted to mean that during the excitation of a nerve there is an increase both of positive or katabolic and negative or anabolic work. The positive work predominates, especially if the nerve is in the asthenic condition from poor nutrition or cold. The fact that stimuli are retarded and weak ones, entirely suppressed on passing through the spinal cord is evidence that the stimulation of normal central cells causes an increase, especially of the anabolic process. Poor nutrition, exhaustion, cold, and various drugs, especially strychnine, produce the asthenic condition, in which the anabolic process is relatively weakened and the katabolic increased. The fact that nerve conduction is only in one direction through the central cells and that interferences of stimulation take place, gives the foundation for the assumption of regions in the central cells, in one of which the positive work predominates, in the other, the negative. The stimulation of one region causes its characteristic work to spread over the entire nerve cell, so that inhibition or excitation takes place according to the region stimulated. Here as well as in the other cases the asthenic condition diminishes relatively the inhibitory or negative work. Stimulations which would inhibit in the normal condition, in this, produce reënforcement. The fact that reënforcements of sensations and muscular movements are more prominent in neurasthenic and hysterical persons seems to be paralleled here. The greater excitability and sleeplessness in excessive fatigue are another illustration.

The activity of a nerve is a function of both the inhibiting and exciting influences, that is, of the anabolic and katabolic processes. The greater the energy of a nerve, the greater both the negative and the positive work; both of which diminish with exhaustion, especially the negative.

The results of the activity of the nervous system upon its own condition have been grouped under the summation, facilitation and diffusion of stimuli, practice, fatigue and habit. The object here is to refer to these facts simply as a means of explaining some of the daily variations observed. A series of induction shocks applied to the posterior roots of the spinal cord or to the cortical centres may produce a contraction by the summation of effects in the nerve centers when a single shock of the same strength is too weak to have any result. A reflex produced by the stimulation of a given sensory nerve will be facilitated or strengthened if, shortly before, a contraction of the muscle has been produced by the

stimulation of this or any other sensory nerve. The familiar "warming up" to work is probably to be explained in large part by these facts of nervous activity.

In the reflex animal a weak stimulation of a sensory nerve causes a contraction of the muscles of the same side ; a stronger one, of those of the opposite side at the same altitude ; a still stronger one, a contraction of the muscles lying higher and lower, but predominantly on the side stimulated. A nervous process set up anywhere in the spinal cord tends to diffuse itself in all directions. Similar facts are brought out by experiments upon men, though for this purpose hysterical patients give the most striking results. Féré has shown that the muscular power measured by the dynamometer is greatly increased by previous movements on the same side, and slightly increased by movement upon the opposite side. Any mental activity augments the muscular power on both sides. Musical sensations have a dynamogenic power in proportion to their intensity and height. A similar dynamogenic scale may be made of colors, beginning with red and ending with violet. Sensations of taste and smell have a similar power. An increase of the discriminative sensibility for colors was also noticed as the effect of mental effort or sensory stimulation. Not only were the energy and speed of the movement increased, but also its endurance. Fatigue brought on by too long exposure to a color produced contrary results. Féré notes that reaction time does not reach its greatest rapidity till the stimulating influence of light and heat has operated for some time. Nocturnal paralysis and morning tire are explained as exaggerated phenomena of this sort. The blood distribution is in rapport with these increments of power. These facts, the reënforcement of minimal sensations by other sensations, the influence of central nervous processes upon the knee jerk and the sweat glands, the slight unconscious movements which accompany attention, and many familiar facts of the influence of mental states upon respiration, circulation and secretions, show that central nervous processes diffuse over other centres. If fatigue sets in, there seems to be a corresponding depressing influence diffused over the nervous system. The diffusion of the effects of nervous activity and their retention and summation by physiological memory are the chief facts on the nervous side, which seem to account for the increase of the rate of certain processes with hard or exciting mental or physical work. The increase of power by practice is probably also connected with these facts.

The tendency of work is thus to increase in amount till exhaustion takes place. It is a common experience that the more mental work is done, the more can be done up to a certain limit, beyond which come collapse and despondency and other symptoms of over-training.

While the nerve fibre is comparatively independent of oxygen and food supply, as is shown by the fact that it will function either in air or in indifferent gases for a considerable time after excision, the metabolism of the nerve cells is very rapid. In the experiment with Bretino, Mosso found that unconsciousness and convulsions were produced by only a few seconds' compression of the carotids. The hole in Bretino's skull made the reduction of the blood supply easy, since this was not opposed by the atmospheric pressure. The muscles of the arm still respond to the will after the blood supply has been shut off for half an hour. This shows that nerve cell metabolism is much the more rapid. Dr. Corning has also experimented with compression of the carotids to reduce hyperæmia,

and study the effect of diminution of the blood flow upon mental processes. Compression of both carotids was followed by facial pallor, drooping of the eyelid, dilation of the pupil, soporific tendency, dizziness, heaviness and confusion of ideas, and finally by syncope. He notes that dizziness and confusion of ideas come more quickly if the compression was made toward evening than if made in the morning, which points to fatigue of the brain cells at evening. The latter result would probably not be generally true. Account should be taken of the different rhythms of mental activity, which, as has been shown, is not a problem capable of a general solution.

The great influence of qualitative changes of the blood stream upon nervous activity is well known. Asphyxia can be produced by suspension of breathing for a few minutes. The poisonous atmosphere of crowded rooms produces headache and various other nervous troubles. Especial interest attaches to the qualitative changes of the blood in connection with the theory of fatigue. The poisons of various contagious fevers are probably the waste products of various bacteria. The activities of the cells of the body similarly produce waste products which are more or less poisonous. Washing a fatigued muscle will restore it. Mosso found that the injection of the blood of a tired dog into the circulation of a fresh dog produced in the latter all the signs of fatigue, while a similar injection of blood from a fresh animal had no such effect. Mosso attributes the decrease of muscular power after the four or five hours' intense mental work in the experiment referred to above, as due to the poisonous effect of brain work. Choline and neurine, decomposition products of lecithin, one of the chief constituents of nervous tissue, which may be formed in cell metabolism, have a poisonous effect like curare. Xanthocreatin, which appears in physiologically active muscles, produces depression and excessive fatigue. The presence of uric acid in the blood produces marked symptoms of mental depression and irritability. According to Haig, the neurasthenic symptoms of morning tire and depression after meals are to be attributed to its influence. Its presence depends upon the alkalinity or acidity of the blood. If the blood is alkaline, it removes uric acid from the tissues; but if acid, it causes its storage. By making the blood artificially acid or alkaline, symptoms of depression or exaltation can be produced at will. He assumes that there is an alkaline tide in the morning, after meals and in the spring, to account for depression at those times. In this connection, it is of interest to compare the psychometric investigations of Kraeplin, Münsterberg and others on the effect of drugs upon the mental processes. Experiments with the ergograph upon muscular power have usually shown that the effect of meals was stimulating. In the experiments reported in this paper, three subjects showed a decided stimulating influence of meals, but one showed as marked a depression from them. The effect is probably not a constant one for the same person, but varies with his power of digestion. In the experiments by the writer in Table IV., the stimulating effect of dinner is well marked. In another series of experiments, extending over fifteen days, but not reported here, a depression after meals was equally well marked, while the general daily curve was otherwise the same.

The term fatigue includes a number of facts, not all of the same kind. Its fundamental idea seems to be diminution of power from excessive work. It refers also to the painful feeling accompanying such work. Sometimes it is applied to the decrease of rate of work. If the energy, rate of work, and feeling of fatigue varied concomi-

tantly, so that one could be taken as the symbol of the other, this use of the word would be convenient. In the experiment of C. H. J. there was a severe feeling of fatigue at 12 M., but no corresponding decrease of rate of work. If energy is measured by the endurance still possible, this evidently diminished from the start. In the so-called second stage of fatigue, there is fatigue anæsthesia, a high rate of activity, but an actual store of energy less than in the preceding state of normal fatigue, as is shown by the after effects of the experience. The true physiological condition is not, however, open to direct inspection, but must be inferred from the blunting of the sensibility, the failure of mental processes, and the accompanying feeling of pain, together with various physical signs, such as loss or gain in weight. The rate may not run parallel with the store of energy, but may be ahead or behind, according to various stimulating and depressing causes. According to Cowles, mental symptoms are the most sensitive accessible indices of exhaustion. The order in which they appear is depression of spirits, decrease of voluntary control, morbid introspection, and finally, diminished sensitiveness.

The curve of fatigue calculated by Dr. Hodge from the shrinkage of nerve cells shows that fatigue is first rapid, then slow, and again rapid under continuous stimulation. The curve of recovery is symmetrical with it. He compares this with the somewhat similar curve of muscle fatigue obtained in experiments with the ergograph, where central rather than peripheral fatigue is really involved in many cases.

Bowditch and others have shown that nerve fibres are not easily exhausted. The sense organs differ greatly with respect to fatigue. The sensibility of taste, smell and sight falls off rapidly under stimulation. The retina seems also to undergo daily changes. To two observers, objects seemed twice as bright in the morning as in the evening. Fatigue of the ear comes on more slowly, but has been demonstrated for high tones and loud momentary sounds. Local fatigue for certain tones has also been shown to occur. Stumpf thinks his discriminative sensibility for tones is greater in the evening than in the morning; but as this depends upon the mental processes rather than upon the condition of the ear, this may be due to the fact that his mental activity increases towards night. A continuous series of discriminations of pitch from morning till night, with an intermission for dinner, was made by H. K. Wolfe to study the effect of fatigue upon the number of right cases. In spite of painful fatigue, no diminution in the number was observed. It is to be noticed that he was in good training for this kind of work. Within certain limits, fatigue for a sense organ does not involve a diminution of the power to discriminate. Fechner calls this the parallel law, because it is to be looked upon as the application of Weber's law to inner physiological changes. His experiments have already been referred to. v. Kries found that two lights appeared the same relatively, whatever the fatigue.

That the daily rhythm of mental activity is much influenced by habit is a familiar fact. A brief habituation to certain hours of sleep is sufficient to leave a tendency to sleep at that time. The subject of Experiment VIII. had been accustomed to a short rest in the middle of the forenoon, the month before the experiment. The effects of this seem to show in the records, the average of which for the first three days are 18.3, 20, 26.3, 15.3, 12.7, 21.3, 22.7, 22.3. There is no evidence of its lasting longer. This influence is in part analogous to that of certain eccentricities of eminent men.

Many writers feel the need of being in particular spots, of using peculiarly colored paper or ink before they can do well. Rousseau found composition difficult unless he was walking. Neander composed best lying on his stomach. Coleridge liked to compose when walking over uneven ground. Sheridan composed at night with a profusion of lights around him. Lamertine had a studio of tropical plants. Dr. George Ebers imagines himself more at liberty to write with a board on his lap than at the desk. Vacano composed at all times, but the place he was in was important, and he could write best in the hubbub of peasant life near an old mill. Maurice Jokai must have fine pens and violet ink. These habits, however acquired, evidently have great power of distracting the attention if they are not satisfied, and so retard work. As a positive influence, they may serve as a sort of hypnotic signal for the state of composition. Similarly certain times, certain occupations may serve as a signal for rest or activity during the day. The increase or decrease of mental excitement may thus be due to no special physiological change, but to the influence of suggestion and habit, and they are factors which should be taken into account in mental or physical training as well as the more prominent physiological facts.

Other Experimental Work upon the Subject.—In his experiments upon memory, Ebbinghaus found it required twelve per cent. more time to learn a sixteen syllable series from 5 to 7 P. M., than from 10 to 12 A. M. Oehrn notes incidentally a single trial of one hour in the morning and one hour in the evening by two subjects, one of whom added faster in the morning than in the evening, while the other did the contrary. Dr. W. P. Lombard, in experiments with the ergograph, found a remarkable twenty-four-hour periodicity of the power of making voluntary muscular contractions. Both in the experiments upon the causes of variation in the knee-jerk, and in these just referred to, a marked influence of barometric changes was noticed. Rising barometer was followed by an increase, falling barometer by a decrease of muscular power. The actual barometric height was unimportant. The constant daily variations had their maxima at 10 A. M. and 10 P. M., and minima at 4 P. M. and 4 A. M., thus corresponding with the constant barometric changes. Daily variations from the constant curve followed the accidental fluctuations of the barometer. While there is a little evidence that two subjects were influenced by barometric changes in the present investigation, there is none for such a daily periodicity. In his case, the effect of the slight regular changes of pressure is supposed to have become organized into the habit of daily variations referred to. Dresslar found a daily rhythm in the rate of tapping which seemed to correspond with his habits of work as teacher for the two previous years. The rate was increased by exciting mental work, but diminished by long walks.

PART II.

Some experiments upon memory by means of the interference of associations were reported in the AMERICAN JOURNAL OF PSYCHOLOGY, Vol. V. No. 3. The object here is to give an account of a few additional experiments and especially to show the influence of interference upon mental activity. Table X. gives a summary of the results of the article referred to. The memory experiment consists of sorting two packs with the same words or symbols successively into different positions. On the average sixty-five seconds were required for the first pack, but eighty-five for the second, if

this was sorted immediately afterwards. The difference is called the interference time, since it is due to conflict of associations and not to fatigue. The time for sorting the cards decreased considerably with practice, but the amount of interference did not. The average of the interference time for the first four subjects, the first seven or eight days, is 17.10 seconds, for the next seven or eight days, 17.53 seconds. This shows that it is not a temporary phenomenon which a little practice may obviate. The interference time is not due to fatigue, since, if two packs with different symbols are sorted in succession and the order in which they are used is changed to compensate for differences in them, the average of the first pack for four subjects is 62.89, for the second, 61.99, showing no increase. If there were general fatigue of the attention or of the nervous susceptibility, that would show itself by an increase of the time of the second pack with different words. The effect might be due to local fatigue of a striking sort; but three facts set aside this hypothesis. Less time is required for the second pack if the cards are sorted into the same places as before. In sorting pack after pack with the same symbol continuously for an hour or so, the time of the first is short, that of the second long, that of the rest is about the same as the time of the second, showing only toward the end a slight tendency to longer records, which may be due to fatigue. The chief fact which shows the nature of the process is the very

TABLE X.

	3″	15″	30″	60″	120″	240″	480″	960″	Number of Experiments.
F. D.	26.62	21.59	20.69	21.08	20.11	15.88	13.56		112
P. E.	±1.23	±1.25	±1.5	±1.45	±1.18	±.96	±.97		
O. C.	17.33	13.48	14.18	13.45	10.66	11.72	11.34		126
P. E.	±.91	±.74	±1.17	±1.26	±1.26	±1.25	±1.18		
M. E. B.	28.79	23.66	19.89	17.34	13.57	13.09	10.08	7.79	94
P. E.	±.70	±.92	±1.20	±1.01	±1.31	±1.23	±1.62	±1.11	
J. A. B.	33.09	25.18	20.42	15.76	13.75	12.	11.04		147
P. E.	±1.2	±1.07	±1.1	±.83	±.81	±.65	±1.04		
T. L. B.	21.85	21.42	17.88	12.25	10.02	11.2	10.25		66
P. E.	±2.28	±2.19	±1.87	±2.14	±2.02	±3.96	±1.44		
Average	25.54	21.07	18.61	15.98	13.62	12.78	11.25	[7.79]	

great tendency of some subjects to make false motions in the direction of the places the cards were in for the first pack. The delay can be seen to be caused by the actual making of a great number of incorrect movements. To a large extent interference is unconscious. With the longer intervals where the interference amounts to twenty or twenty-five per cent., the subject frequently feels no trouble. With the shorter intervals there is usually great confusion, but the false movements are not known to be such till they have been made. This shows the reflex nature of interference. The sorting of cards involves the learning of new associations and requires an intense effort of attention, any distraction of which causes a great lengthening of time. Interference demonstrates experimentally certain relations of mental activity and memory to the nervous system, since its persistent, involuntary, reflex nature proves it to be physiological. It shows that even such complex processes after a very few repetitions are carried on largely by reflex activity; and that reason comes in chiefly in case of error. The first horizontal row gives the intervals between the two packs. The following rows give the amounts of interference for the different subjects as the interval increased, together with the probable error.

The general feature is a rapid decrease of interference at first, with a very slow diminution afterwards. The attitude of the subject is to forget rather than to remember the previous positions.

Interference in the other memory experiments has only been studied upon the writer; 226 series of nonsense syllables, ten each, were learned from March 20th to March 31st. Four series were memorized each time, with about ten seconds' interval between to give an opportunity to mark down the result. A similar experiment was made with number series, each containing thirty digits. Three series were learned each time. There are eighty-eight in all. The average of those which were learned in the first, second, third and fourth places gives the following result:

	1	2	3	4
Syllables.	104.55	131.05	134.78	137.16
Number.	271.75	302.12	306.96	——

The time of the second series is considerably longer in both cases, but the increase in time after that is very slight. A similar result is found in Ebbinghaus' records. The average of ninety-two groups of eight twelve-syllable series gives for the successive series 105, 140, 142, 146, 146, 148, 144, 140. The great increase of the second above the first and the slight difference afterward is noticeable. The same difference between the first and second is to be found in his other experiments. The series whose averages are given was taken at the beginning of his experimental work and shows a slight increase of time up to the sixth, which may be due to fatigue. In subsequent experiments there is no such increase, but after the great lengthening of time for the second series, there is a certain oscillation above and below the average with no evidence of fatigue. Ebbinghaus notes especially the rhythmic oscillation of the averages. The odd series were learned more quickly than the even. He attributes it to a rhythm of the atten-

tion or the sensibility, but does not explain the fact further. The interference of association will explain the sudden increase of the time of the second and probably also the rhythm of the odd and even series. As has been said, nonsense syllable series are essentially different arrangements of the same symbols. The opportunity for interference thus exists. To test the matter experimentally, nonsense syllable and number series were made up from the first half and the last half of the alphabet and digits respectively. Nonsense syllable and number series could now be learned in succession without interference. If the lengthening in time took place nevertheless, fatigue might be the cause. Series of nonsense syllables and numbers were also learned alternately, thus shutting out interference. The result shows that when interference is avoided, no increase of time takes place. The fact that reading written and printed letters may be learned separately, gave rise to the theory that if the second series was written and the first was printed, or vice versa, interference might be avoided, but this was not verified. Interference will therefore explain the increase of time of the second. It is also an influence which is fitted to give rise to the rhythm noticed by Ebbinghaus. The third series is probably learned more quickly than the second, because the interference from the first has died away, and the second series was not learned so well and does not retard so much as the first did the second. Since the third is learned more quickly, the interference again becomes greater for the fourth, so that if the oscillatory variation is set up it would tend to perpetuate itself.

Two series of numbers and two series of nonsense syllables, one of each kind described, were learned at the same hours as the series last referred to. Sixty-six series of nonsense syllables and sixty-eight series of numbers give the following result:

	Syllables.		Numbers.	
	1	2	1	2
Average.	151.	138.99	228.34	193.29
P. E.	±8.21	±7.14	±10.61	±6.13

1 and 2 in the second row indicate that the series was learned in the first or second place. Fourty-four experiments, in which a syllable and a number series alternated in the first and second places, give 172.75 and 173.65 as an average. There is an actual decrease of time for the second series of numbers, while the averages of the alternating series are about the same. The experiments with the series give, of course, only an individual result, and chief reliance is placed upon the experiments of others with the cards.

The following explanation of the decrease of the time of the number series may be offered. The subject had been learning series in which all the digits appeared, for considerable time. Series containing half the usual number may be supposed to call up the absent members. There would be simply stimulation of the nervous tracts, but no formation of associations. The effects would summarize and the series containing the absent digits be more easy

to learn. The same thing is noticed in the syllable series to a less extent. If packs of cards are sorted in immediate succession the results resemble those for learning a number of series. The averages of a series of card experiments, on five different days, by F. B. D., were 113.2, 140.6, 135.4, 144.8, 148.8, 140, 139.4, 143.2, 138. The increase of the time of the second and the oscillation of the rest above and below the average are to be noticed.

That the interference is not due to any local nervous association of the centres of the eye and hand, but is the after effect of a more central or apperceptive mental process, is shown by the following experiment. Instead of actually sorting the first pack, and thus learning the associations which afterwards retard the second, the subject is asked to learn them by repetition, like nonsense syllables, till he can tell where they are. This excludes special training of the hand centres, and in case the positions are learned by ear, of the eye centres. Had the interference disappeared for sorting by hand, that would be evidence that it was due to some local association; but the fact that it appears strongly shows that the nervous process is central. Table IX. gives the results of the experiment. Under "before" and "after" is given the time for sorting a pack of cards without interference from a previously learned pack. Under "eye" and "ear" are given the time required for sorting a pack when different positions of the cards have been learned previously by seeing them on the table or by being told where they were. Under "eye" and "ear" the numbers are averages of three, so that the final average is made from twelve experiments. Different packs were used "before" and "after" on different days, to compensate. The experiment required nearly an hour each time, but it will be seen that there is no evidence of fatigue. The fact that the interference is greater when the eye is used to learn the positions on the table, is perhaps connected with the fact that the positions were learned more easily by eye than ear, the average time required being 78.9 and 126.6 seconds respectively. Some time was, of course, lost through the person who told the positions in learning by ear.

TABLE XI.

M. E. B.

	BEFORE.	EYE.	EAR.	AFTER.
	64.6	85.33	79.33	56.6
	55.5	87.53	77.73	58.4
	56.4	83.66	80.09	54.2
	51.8	88.	77.8	56.
Average.	57.05	86.13	78.76	56.3
Interference.		29.45	22.02	

After-images of all sorts are of special interest for the theory of the relation of mental states to the nervous system. The possibility of the experimental variation and measurement of interference, which may be called an after-image of central activity, makes it important in this respect. After-images and practice give, in a way, more minute and tangible, if not so full, evidence of mental and nervous concomitance as the great lines of argument from the mutual influence of bodily and psychic states, brain lesions, and comparative anatomy and psychology. The last experiment shows that similar impressions, in so far as they are identical, go to the some central tract, irrespective of what sense they were learned by. It emphasizes the importance of the central associative nervous process, and is opposed to any rigid dismemberment of the memory for facts into different sensory types, except where by this distinction is meant that the same thing can be learned more easily by one sense than by another.

With reference to the rate of mental processes, the fact brought out by these experiments is that if certain sensory data have been associated in one way, it is temporarily more difficult to make a different association of them than if the first did not exist. Table X. shows the rate at which this difficulty diminishes with time. If the interval between the two packs is twenty-four hours, the second will be sorted more quickly instead of more slowly, as is shown by the steady increase of the rate of work by practice. There are two opposing tendencies in the experiment—one, the general training of the attention and the organization of all the movements; the other, the intense associations just made by learning the positions in any given experiment. The latter is temporarily able to produce the retardation noticed, but, since it is the result of only a few repetitions, it fades away quickly, while the first is continually strengthened by practice. The resultant of the two tendencies thus makes the sorting of the second pack or the learning of the second series immediately afterwards much more difficult, but, after a considerable time, easier. The application of the fact to memory experiments and memory work generally is, of course, evident.

It has probably, however, more important applications to inventive mental work. For this view there is no direct experimental evidence, but only such general reasons as will be adduced. The influence of physiological memory in retarding the progress of thought is in some respects recognized. After reading a certain author, many persons find it hard for a time to assert their own stand-point or style. It is usually necessary to let new views sink down to the level of intensity of older views before a fair estimate of them can be made. Many have been obliged to form the habit of letting matters which arouse their interest rest over night for a fair judgment. In mathematical calculations, many persons will repeat the same error over and over again, and be temporarily unable to obtain a solution. After a few hours they return to the work and are surprised at the ease with which they arrive at the correct result. While the memory of the error was recent, it prevented the true association; but when its intensity had diminished sufficiently, the correct association again became easy and natural. Similarly, in the translation of a foreign language, the mind may have become set for a certain meaning, or some error may have crept in and the passage is for the time misunderstood. Some hours later the proper sense of the passage may appear at a glance. In investigation, there may be a clear knowledge of the end desired and the power to estimate the value of facts and theories which

bear upon the problem; but the solution is conditioned by the association of ideas. In invention of all sorts, mental processes form with difficulty, and slight hindrances are sufficient to obstruct them entirely. Many of the processes which belong to the solution of the problem do not enter distinct consciousness at all, and yet may be essential for a correct result. These are especially liable to error. Everyone has perhaps heard long debates upon such questions as: "Which should we say, that five and six *is*, or that five and six *are* twelve?" With the attention focused upon *is* and *are*, the wrong association, five and six equals twelve, is not noticed. A mathematician says that in studying geometrical figures, only a few relations can be made out at a time. Others are seen so easily some time afterwards that the wonder is they were not seen at once. In this case, one grouping of the facts produced a certain discovery which gave a given direction to interest and association. Other associations, not in the same direction, would be temporarily more difficult. With this, there may co-exist certain errors like those in computation and translation, whose intensity must die away before progress can be made. In recalling names or facts which are nearly forgotten, we sometimes succeed at once; at other times we find it impossible. A second trial a few hours after our failure may bring success at once. The resultant influence of recent experience perhaps sent the associative movement in the wrong direction. This error would still further increase the difficulty. Both influences must diminish in intensity before the correct association can take place. A persistent hunt for clues is fatiguing, but, of course, often succeeds.

As a class, these facts have usually been explained by some theory of unconscious cerebration, or they have been attributed to the summation of stimuli, or to rest.

That these phenomena are due to a summation of stimuli which gradually gathers sufficient strength to break through the nervous resistance into the correct association, seems improbable, since every nervous excitement diminishes in intensity if left to itself, and the quickest and most striking results are usually obtained by putting the problem entirely out of mind for a while.

Fatigue and rest have, as is well known, a considerable influence upon the rate of mental work, but these cases seem to occur, just as in the memory work, when there is no fatigue present. The unreliable and often fantastic character of mental processes in the indirect field of consciousness, in revery, and in dreams, the stupidity of secondary consciousnesses, together with the absence of fatigue from this imagined unconscious cerebration, makes it probable that it is of little importance, and that conscious attention is the forge in which most, if not all, valuable mental work is done.

It is not believed that the formation and fading away of certain errors and tendencies of association offer a complete explanation of these and similar facts, but simply that this influence should be given a place in any list of those which hinder mental processes, and that it is a prominent cause of the surprising retardations in the cases described.

I.

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III.

To the references given by the writer for the memory experiments in the AMERICAN JOURNAL OF PSYCHOLOGY, Vol. V. No. 3, must be added:

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